

small groups at the lines of union of the membranes, which are extended in the vestibular space.

Halme globosa, R. v. L. The sense-cells stand in groups at the edges of the membrane which are extended in the lacunar spaces of the afferent canal-system.

Euspongia canaliculata, R. v. L. The sense-cells form zones which surround at the surface the lacunar dilatation of the efferent canal-system.

These isolated observations do not enable us to draw any general conclusion, as the observations on the various species are so very different.

I have already indicated * that Schulze's denomination of the contractile elements as "contractile fibre-cells" is no longer necessary, and may now be replaced by the designation "muscle-cells," seeing that nervous elements have been found with them.

Both the muscle- and nerve-cells are *mesodermal*. The *epithelia* of the Sponges nowhere appear to be further developed after the fashion of the higher Cœlenterata. Both endoderm and ectoderm always remain simple †.

XXXIII.—*A few Remarks on Mr. Butler's Notes on the Genus Terias.* By W. L. DISTANT.

IN the last number of this Magazine I have read with no inconsiderable interest a paper by my friend Mr. Butler, entitled "Notes on the Genus *Terias*." In this communication the author, after expressing the very sound opinion that "it is quite impossible for any one, in our present profound ignorance of the earlier stages of most of the species and our imperfect knowledge of those of all, to lay down the law as to which of these forms is worthy of a distinctive name and which not," has still been compelled to describe *twelve* new species, and also to again do me the kindness of not only reviewing some of my recent work, but also to contribute much readable criticism thereon, and to offer many alternative suggestions for my consideration. Under these circumstances I have felt it would be discourteous to any longer refrain from affording such explanation as is possible to one who has taken the trouble to read my remarks, and also I have considered it necessary to myself to show that the views I had the temerity

* Zoologischer Anzeiger, no. 186.

† Hæckel has described some Calcisponges with locally plurilamellar endoderm, but hitherto this statement has not been confirmed. See also Vosmaer in Bronn's 'Klassen und Ordnungen des Thierreiches: Porifera.'

to state remain entirely unmodified, but rather strengthened, by the criticism which Mr. Butler has presented.

I will further preface my remarks by an assurance to my friend that I am actuated by no splenetic motive, that I have no belief that anything that can be adduced or argued can now alter or modify the various specific dogmas with which his name is, and will be, indissolubly united; nor do I, on the other hand, imagine that such criticism as he has afforded on my behalf is less friendly than that which he has already presented to most of his contemporaries. At the same time, I cannot disguise the fact that in following Mr. Butler through any monographic paper which he has written, I have seldom failed to have the misfortune of disagreeing with some of his specific discriminations, and have sometimes not hesitated to publish my dissent from the same.

The opening of this *Teriad* campaign will be found in the 'Annals' for 1885, vol. xvi. p. 336, where, after some few paragraphs, the following peroration is reached:—"However I am willing to accept his admission—a rash one for an entomologist to make—"I treat this species as a variety" (see p. 321). I know of many lepidopterists who do this; but Mr. Distant is the first who has boldly come forward and confessed it."

Now this formidable quotation is a statement which, I am glad to have an opportunity of stating, still in every way exactly expresses my views. The meaning is very simple and very clear. Boisduval described what I consider a form of *Nepheronia hippia* as a distinct species under another name, and I could only write, "I treat this species" (meaning Boisduval's creation) "as a variety of *N. hippia*." Mr. Butler has described many "species" (probably even exceeding the number of those of Walker), and naturally not a few of these have been, and are constantly (though termed "species"), treated by some entomologists as simple varieties of other species. Hence I am afraid I cannot accept Mr. Butler's great compliment of being "the first who has boldly come forward and confessed it."

We now come to the question of the value of "types," and how far a species shall be regarded as defined by a description without an actual examination of the specimen described. A figure hitherto has been considered decisive, especially when drawn by or under the hands of the original describer. But now a new case is cited by Mr. Butler, for which no precedent exists. In 1869 he described a species under the name of *Thyca ithiela* as from Penang. In 1871 he figured it under the name of *Delias ithiela*, still

giving the same habitat, and repeated the information in a third organ of publication in 1872. I reproduced the figure and description in my 'Rhopalocera Malayana,' the habitat "Penang" compelling its insertion. We are now informed that the locality was an error, as the type was labelled thus: "P.," which with Wallace's specimens denotes "Penang," but with specimens received from the East India Company represents "Darjeeling, Pearson." It is now opined that it came from the last-named locality, and it is stated that "had Mr. Distant examined my type, which, by his own admission, he did not do, he would have avoided the repetition of this error." Surely this logically implies two axioms, viz. (1) Mr. Butler's recorded localities cannot be taken without an examination and verification of the labels attached to his "types;" and (2) if "types" are not contained in this country, neither names nor localities should be used.

" *Terias senna*, Feld."

Mr. Butler states that I have figured what he considers and described as a distinct species (*T. inanata*) as the *T. senna*, Feld. If this is so, then I appear to have erred in describing *T. inanata* as a variety of *T. senna*, and I should more correctly have treated it as a simple synonym of that species. I examined Mr. Butler's "species" in the national collection before I wrote, and, though words may be found to represent differences, I certainly failed to see any exhibited in the specimens themselves that appeared to warrant their differentiation. Mr. Butler quotes Felder's differential diagnosis between *T. senna* and *T. santana* with approval. If these are distinct, why did he in another Teriad paper, published in 1871, enumerate *Terias senna* as a variety of *T. santana*? If, however, we turn to Mr. Butler's original description of his *T. inanata*, we read that it only differs from other specimens which he described under the name of *T. hebridina* by "the entire absence of markings on the under surface of the wings." Now I have figured two specimens of *T. senna*, one with markings underneath and one with those markings absent; and therefore if, as Mr. Butler says, I have in this way figured his species, then his description must be wrong. Again, he has figured this *T. hebridina* (P. Z. S. 1875, pl. lxxvii. fig. 8), from which he says his *T. inanata* does not differ on the upper surface; and surely "every candid reader" to whom he rightly appeals must be struck with the dissimilarity between that figure and those given by myself. Probably some explanation was inadvertently omitted.

“*Terias æsiope*, Mén.”

Mr. Butler remarks that I have figured as this species a male variety of *T. hecabe*, which is quite true, and it is strange that though this is considered heterodox to-day, he wrote of that species himself, in a former Teriad paper, “Probably a form of *T. hecabe*.” I now come, however, to a less pleasant statement, and one which Mr. Butler must be the first to acknowledge as of a misrepresentative character, when he affirms that, amongst other localities for this form, I have given the “somewhat wide one of continental India.” The habitat I gave is “Continental India; Bombay.” It would surely be quite as correct to say that the “somewhat wide one” of Mexico is given in the ‘*Biologia Centrali-Americana*,’ because the primary division is there given before the smaller habitats which it comprises. I did not imply that Mr. Butler was unaware that Bombay was in continental India, but only followed the usual monographic method of giving the habitat of the species. Mr. Butler speaks of the “true *T. æsiope*;” but surely this must be difficult to define, as the species is clearly varietal on his own authority (Trans. Ent. Soc. 1879, p. 7). He there also states that the species has been received from Cachar, N.E. India; but he now implies that it is confined to China, Formosa, and Hainan.

“*Terias sari*, Horsf.”

Mr. Butler doubts that I have correctly figured the typical form of *T. sari*, as I have affirmed, and thinks “it far more likely” that a Bornean male specimen in the British Museum is typical of the species. I did not make this determination upon any opinion of my own, but from a comparison with a specimen labelled typical in the collection of Mr. F. Moore, and upon the authority of that lepidopterist, who, as is well known, was once intimately associated with the work of Dr. Horsfield. Mr. Butler can easily examine that specimen for himself, for it is in the collection of a mutual friend, at whose house we have spent many pleasant hours together, and to whom both he and I are indebted for much information regarding oriental Lepidoptera.

I now take leave of a discussion which possesses little scientific value. My friend Mr. Butler holds the proud position of being delegated to look after the national collection of Lepidoptera, and seeks conscientiously to fulfil his duty by industriously describing and naming the specimens placed

under his charge. If others, beside myself, hold a sceptical opinion as to the universal efficacy of this operation, we have at least the satisfaction of knowing that the "types" are contained where they can be examined, and where possibly at some future day a few of them at least may be relegated back to what we are heretical enough to think is their more proper position.

BIBLIOGRAPHICAL NOTICE.

Evolution without Natural Selection ; or, the Segregation of Species without the Aid of the Darwinian Hypothesis. By CHARLES DIXON. Small 8vo. London : R. H. Porter, 1885.

IN all matters of opinion, in politics, philosophy, and religion, we find the partisans of one view or the other in possession of certain cabalistic terms or phrases which are supposed by them to settle all difficulties. It would be hard upon the naturalists to be without a shibboleth of this kind, and accordingly since the publication of Mr. Darwin's 'Origin of Species' the term "Natural Selection" adopted by that great naturalist has been freely employed by a great number of his followers as a formula of this nature. In all questions relating to species and their affinities disputes were considered to be closed by the use of this mysterious expression, and it is no doubt in opposition to this employment of the term "Natural Selection" that Mr. Dixon has produced the little book of which the title stands at the head of this article. Unfortunately, however, the author does not seem to have realized more clearly than his presumed opponents the precise sense in which the phrase was used by Darwin. From the whole construction of the volume on the 'Origin of Species' and the line of argument followed in it the meaning attached to the term in the mind of its originator is perfectly clear. Starting from the demonstration of the production, in the case of domestic animals, of a set of varieties so widely differing in character that if met with in nature they would certainly have been regarded as distinct species, these extreme varieties having been produced by the deliberate selective action of man, taking advantage of comparatively small accidental differences, Darwin proceeded to show that an analogous process may very well have occurred in nature, and being, in the lapse of time, carried even still further, may have given origin to true species in the physiological sense of the term. And "artificial" or "methodical selection" by man having been shown to be the cause of the great variations in certain domestic animals, he somewhat metaphorically employed the term "Natural Selection" to express the sum of the actions upon which he considered the origin of still wider variations in nature to depend. But Natural Selection in the Darwinian